severe aortoiliac occlusive disease. This claim is supported by comparison of the mean total hospital cost ($10,585 for graft versus $9,161 for stent *per limb affected*). First, as one of the experts in the discussion panel noted, the cost of surgery is probably underestimated. Second, the authors reported cost per limb affected rather than per patient treated. Eighty-seven percent of the patients in the surgery group and only 15% of the patients in the stent group who underwent treatment had bilateral disease. This nonrandomized assignment of patients with bilateral disease favoring surgery adversely impacts on the cost for the stent group. If more patients with bilateral disease had stent deployment rather than surgery, the cost for the stent group would be markedly reduced. By the current assignment of most patients with bilateral disease to the surgery group, the authors essentially reduced the cost of surgery by 50%. It would be important to know the number of stents used per case because this information would have an impact on the cost. For example, instead of using two stents to treat a long lesion, one long stent would lower the cost of the procedure.

Furthermore, I disagree with the statement that the cost of the operating room time and the longer hospitalization (10 days for surgery versus 2 days for stenting) for the surgery group did not impact on the total hospital cost. The authors failed to include in the cost of surgery the anesthesiologist's fees, the consultant's fees (to manage the severe complications in the surgery group), and the costs associated with the preoperative workup and evaluation before surgery, which are not required for stent deployment.

Interventional radiologists at my institution place stents for severe aortoiliac disease on an outpatient/ambulatory basis. Patients arrive in the morning for their stent procedure and normally leave the hospital by late afternoon on the same day. Rarely do these patients stay overnight, unless a complication, which is rare, has occurred. In this case, they are admitted overnight for observation. The length of hospitalization for the stent group in the article is higher than in my institution. In summary, the reader should be made aware of the inherent flaws in this article. A randomized, prospective study should be undertaken before conclusions, which impact on the decision of surgery versus stent deployment for the treatment of severe aortoiliac disease, can be made.

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Reply

We appreciate the opportunity to respond to Dr Wang's comments concerning our manuscript.[1] He contends that there are significant flaws in our data presentation and raises some issues that require rebuttal, even though they were clearly stated in the manuscript. We can assure Dr Wang that our data are truly representative of a carefully studied group of patients with severe aortoiliac occlusive disease, and we offer the following point-by-point responses.

1. Our expert interventional radiology colleagues were not included in the author list because they did not assist in writing the manuscript nor were they involved with the data collection for the primary study focus, which was cost. Previous publications from our institution concerning other aspects of aortoiliac occlusive disease clearly give deserved credit to these skilled individuals.[2-4] Editors have taught us all to be increasingly cautious in listing coauthors.
2. All the patients in this series were potential candidates for operative intervention. This is in contrast to other studies that have compared angioplasty candidates with surgical patients.[5,6] Our patients with more severe disease allow the comparison of the two therapies where they are currently competing with each other. In our practice, the treatment for a category I iliac artery lesion is angioplasty with or without stent deployment. Therefore, as was clearly stated in the manuscript, these are nonsurgical cases. They could not be included when only disease categories II to IV were studied. Also, as displayed in the manuscript, there was no selection bias (any significant difference) between the two groups.
3. Regarding a potential patency bias as a result of group differences in infrainguinal occlusive disease, this factor was not significant according to univariate analysis. Therefore, Dr Wang's gross estimate without statistical considerations is in error.
4. To answer another question, one stent was deployed in 45 cases (57%), 2 in 25 cases (31.6%), 3 in 7 cases (8.9%), and 4 in 2 cases (2.5%).
5. The matter of cost analysis is more complex, consisting of a number of items. First is the inclusion of the anesthesia. Professional medicare reimbursement for a 3-hour aortobifemoral (or aortobi-iliac) bypass grafting procedure surprisingly did not result in a statistically significant increase in mean total cost of surgery ($10,830 versus $9161, $P = .172$) and would not have changed our basic conclusions. However, we agree that tabulation of all costs is more appropriate. We also would have liked to include the cost of secondary procedures, but unfortunately including the cost of failed procedures (there were more stem failures) would have been too complicated. We originally tried to include preoperative costs, but this too was complex because some patients referred to the clinic had excellent arteriograms in hand and others had to have better images before a treatment could be selected. We were as surprised as Dr Wang was that operating room time and length of stay data did

not influence the cost to the extent anticipated, or even significantly. We agree with Dr Wang that stents can be deployed safely on an outpatient basis, and this is our current practice. We did not begin our program with that premise, however. On the other hand, the length of stay for direct surgical reconstruction has decreased significanfiy during the last few years. Therefore, these cost data do not fully reflect the ongoing efforts to streamline both procedures. Although we admit that the cost data published in this manuscript are not perfect, they are our best attempt at presenting the data with appropriate cohort comparisons.

6. These cost data were obtained directly from thc accounting office on the basis of accurate patient admission statistics and were not underestimated. They are accurate. The discussant to whom Dr Wang refers was speculating and was not an informed panel expert.
7. As admitted in the manuscript, the cost per patient encounter was higher for surgical reconstruction as compared with angioplasty with or without stenting. However, we believe that the cost estimates derived on a per-limb-treated basis are the best method of comparison and provide a perspective not commonly offered in the interventional radiology literature. That is to say, on the basis of these data, direct surgical reconstruction offered better value per limb treated in terms of clinical improvement and long-term patency rates. Although Dr Wang and other interventional radiologists might contend that this endpoint is unfair because angioplasty was done more for unilateral disease and bypass grafting was done more for bilateral disease, the per procedure or per patient perspective found in much of the radiology literature is unfair in the other direction.
8. It is well recognized that procedural and physician charges for angioplasty and stenting are rising as a result of more sophisticated imaging and instrumentation and that those for surgical bypass grafting are falling because of drastic reimbursement trends. The results of the prospective, randomized trial that Dr Wang suggests, if it includes cost of reintervention and is performed on those patients with more extensive aortoiliac disease than normally treated with angioplasty, might surprise him and his colleagues. We too would welcome it, but until such a trial shows otherwise, we believe that our data reflect a true picture of comparative costs for these procedures when applied to patients with severe forms of aortoiliac occlusive disease.

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Regarding "Detection of active cytomegalovirus infection in inflammatory aortic aneurysms with RNA polymerase chain reaction"

To the Editors

In addition to Dr Tanaka's reply (J Vasc Surg 1998:27:587-8), we would like to provide some additional comments to the critique by Dr Satta and colleagues[1] regarding possible cytomegalovirus pathogenesis in human aortic diseases. We want to avoid misunderstanding because it is slightly complex.

First, we should emphasize that we suggested cytomegalovirus as a possible pathogen of so-called inflammatory abdominal aortic aneurysms (IAAAs)[2] but not of conventional atherosclerotic abdominal aortic aneurysms (AAAs).[3,4] Cytomegalovirus transcripts could be detected only in IAAAs but not in AAAs. Thus, we concluded that cytomegalovirus transcripts might play a role in the sustained chronic inflammatory reaction and massive adventitial fibrosis of IAAAs but did not clearly suggest the pathologic nature of AAAs.

Second, I am afraid that the authors might have confused the status of cytomegalovirus infection in such aortic diseases because the analytical technique that they used does not seem to be appropriate for this purpose. The process of infection of human Herpesviridae is not fully elucidated, but numerous studies suggest that at least four types of conditions seem to exist in cytomegalovirus infection (Table I). The following conditions are suggested: type 1, lytic infection associated with cytomegalic inclusion body that leads to the death of the host cell; type 2, persistent infection with virus production that does not lead to cell death; type 3, latent infection that expresses some viral genes without viral production; and type 4, postinfection with viral genome and without viral gene expression. Clearly, type 1 infection can